

CORRESPONDENCE

Comments on “Incorporating the Effects of Moisture into a Dynamical Parameter: Moist Vorticity and Moist Divergence”

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ABSTRACT

In a recent article, Qian et al. introduced the quantities moist vorticity and moist divergence to diagnose locations of heavy rain. These quantities are constructed by multiplying the relative vorticity and divergence by relative humidity to the power k , where $k = 10$ in their article. Their approach is similar to that for the previously constructed quantity generalized moist potential vorticity. This comment critiques the approach of Qian et al., demonstrating that the moist vorticity, moist divergence, and by extension generalized moist potential vorticity are flawed mathematically and meteorologically. Raising relative humidity to the 10th power is poorly justified and is based on a single case study at a single time. No meteorological evidence is presented for why areas of moist vorticity and moist divergence should overlap with regions of 24-h accumulated rainfall. All three quantities have not been verified against the output of precipitation directly from the model nor is the approach of combining meteorological quantities into a single parameter appropriate in an ingredients-based forecasting approach. Researchers and forecasters are advised to plot the model precipitation directly and employ an ingredients-based approach, rather than rely on these flawed quantities.

1. Introduction

This comment critiques a recent paper by Qian et al. (2015), entitled “Incorporating the effects of moisture into a dynamical parameter: Moist vorticity and moist divergence.” Qian et al. (2015) define the moist vorticity and moist divergence as the well-known kinematic quantities of vorticity and divergence multiplied by the 10th power of relative humidity. The use of these parameters is not justified scientifically or meteorologically. Hence, readers should avoid these parameters, as well as others similar in construction and used in the same way [e.g., generalized moist potential vorticity (GMPV); Gao et al. (2004)]. These types of parameters fail to contribute

to a sound ingredients-based forecasting approach and should not be used in an operational forecast setting. Instead, diagnosis of heavy rain is best done by direct model output of precipitation or through an ingredients-based forecasting methodology such as the one described by Doswell et al. (1996).

2. Their derivation and interpretation of moist potential vorticity is flawed

In motivating the construction of moist vorticity and moist divergence, Qian et al. (2015) reference the construction of moist potential vorticity [MPV, which is the potential vorticity (PV) with the potential temperature replaced by equivalent potential temperature; Bennetts and Hoskins (1979)] and GMPV [which is the PV with the potential temperature replaced by a generalized potential temperature that includes a term with relative humidity raised to the k th power; Gao et al. (2004)]. Qian et al. argue that this approach is “a feasible way to

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include moisture effects within a dynamical parameter.” We argue that their formulation is incorrect mathematically, and its meaning as interpreted by the authors is confusing.

The formulation of MPV in section 3b of Qian et al. (2015) is incomplete. Specifically, they use the three-dimensional gradient of equivalent potential temperature in Eq. (5), but the vertical component of absolute vorticity instead of the three-dimensional absolute vorticity vector, as correctly presented in Schultz and Schumacher (1999, their section 3c). The result is Eq. (5a) in which MPV is expressed as a vector (the product of a scalar and a vector). Had they constructed MPV properly, it would be the sum of three components (not two, although the horizontal components could be combined into a single expression, reducing the number of components down to two). Furthermore, the second component listed in Eq. (5c) is incomplete, as it should also have horizontal derivatives in vertical velocity. Presumably, the authors assumed hydrostatic balance, but that assumption is not mentioned in their article.

Beyond the mathematical errors and missing assumption, there appears to be some confusion as to what MPV measures. On p. 1416, Qian et al. say, “maximum MPV and maximum surface rainfall are nearly collocated as a result of the impact of heat and mass forcing on the development of MPV.” But previously, the authors argued that the benefit of MPV was its conservation even in the face of latent heat release (their p. 1415). Both statements cannot be valid simultaneously.

Such confusion in interpreting the different components continues later in the article. Because MPV is conserved in moist-adiabatic processes, it is not surprising that MPV1 will be compensated by MPV2. Imagine starting with zero MPV and applying heating. MPV will be conserved but the equivalent potential temperature field will change, leading to offsetting changes in MPV1 and MPV2. Thus, their Fig. 5 can be mainly interpreted in this way.

3. Relative humidity to the 10th power is unjustified mathematically and meteorologically

Qian et al. (2015) take the kinematic quantities of relative vorticity (what they consistently shorten to “vorticity”) and divergence—concepts that lie at the heart of meteorological dynamics—and multiply them by relative humidity raised to “an empirical constant varying from 1 to 20” (p. 1417). What is the physical meaning of relative humidity when $k > 1$? The answer is unclear. The specific choice of $k = 10$ in Qian et al. (2015) is based on a single case study of 1 July 1991 to visually “cover all the heavy rain spots” to minimize the fraction of missed events in their Fig. 7. Numerous

places in the text refer to this as “*properly* including moisture effects into a dynamical parameter” (emphasis our own). For example,

“a parameter containing either a dynamic or moisture factor alone, such as vorticity, divergence, or relative humidity, cannot accurately depict heavy rain areas (often leading to too many false alarms), but properly including moisture effects into a dynamical parameter can significantly increase a parameter’s ability to diagnose heavy rain locations” (1424–1425).

The article does not define what is *proper*, why this approach is *proper*, and what the scientific justification for this inclusion is.

After multiplying potential temperature by relative humidity to the 10th power, Qian et al. (2015, p. 1417) conclude, “This new definition distinguishes the relative contribution of saturated water vapor based on its moisture content.” What this sentence means is unclear. Qian et al. (2015, p. 1417) even seem to misunderstand the meaning of relative humidity, writing “the relative humidity of lighter fog could be less than 100% and different from that of denser fog.” No citation for this statement is provided.

4. There is no sound meteorological link between moist vorticity, moist divergence, and heavy rain

On p. 1412, Qian et al. write that these parameters are said to “improve the correlation between areal coverage of the parameter and the observed rainfall location.” Perhaps the question that Qian et al. should have asked is, “Why should a relationship exist between a single parameter and the observed rainfall location?” Nowhere in their article is a meteorological justification for why these areas of moist relative vorticity and moist divergence should overlap with regions of 24-h accumulated rainfall amounts. Any coincidence would appear to be due to a loose relationship between low-level vorticity, divergence, and high relative humidity in precipitating regions. But, this loose relationship is poor justification as an acceptable means of forecasting in this modern age of numerical weather prediction (as we discuss in section 5) and as representing conceptual models of precipitating systems that rely on ingredients-based forecasting methodologies (as we discuss in section 6).

What is the governing equation that links moist relative vorticity and moist divergence to heavy rain or, even more simply, ascent? On p. 1416, Qian et al. write, “MPV1 is a product of absolute vorticity (related to ascent)...”. Absolute vorticity is not related to ascent, but the differential absolute vorticity advection with height is related to quasigeostrophic ascent. If differential absolute vorticity

advection with height is related to ascent through the quasigeostrophic omega equation, then how does *moist* relative vorticity relate to quasigeostrophic ascent? Sure, divergence is related to ascent through the continuity equation, but what about *moist* divergence? Qian et al. (2015) do not provide any insight or resolution to these questions, failing to address the scientific justification for moist vorticity and moist divergence.

Moreover, the validity of moist divergence for studying convective storms has already been criticized previously. Qian et al.'s moist divergence is the second term in the expression for moisture flux convergence (MFC):

$$\text{MFC} = -\nabla \cdot (q \mathbf{V}_h) = -\mathbf{V}_h \cdot \nabla q - q \nabla \cdot \mathbf{V}_h,$$

$$\text{MFC} = \underbrace{-u \frac{\partial q}{\partial x} - v \frac{\partial q}{\partial y}}_{\text{advection term}} - \underbrace{q \left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} \right)}_{\text{convergence term}},$$

where q is the specific humidity, $\nabla = \hat{\mathbf{i}}(\partial/\partial x) + \hat{\mathbf{j}}(\partial/\partial y)$, and $\mathbf{V}_h = u\hat{\mathbf{i}} + v\hat{\mathbf{j}}$. Banacos and Schultz (2005) questioned the operational value of moisture flux convergence for forecasting convection initiation. In particular, these limitations included the different scales that the advection term and convergence term entail, the inappropriateness of MFC for elevated convection, and the superiority of horizontal mass convergence. These same critiques of MFC apply to Qian et al.'s moist divergence.

The rationale for picking the vertical levels on p. 1420 is poor. It seems that the authors are employing a trial-and-error approach for apparently just two levels, rather than a physically based rationale in deciding that, "the performance is better at 925 hPa than at 850 hPa (more misses at 850 hPa by positive PV area)." Indeed, "an optimal threshold (maximizing the [threat score] for a parameter...) is first determined for each parameter based on the 41-case average; then the optimal threshold is used to define the area of each corresponding parameter" (p. 1421). This picking and choosing to obtain the best levels is inappropriate and is likely a result of a poorly motivated meteorological rationale for the parameters in the first place. Moreover, there is no testing of their approach using a dataset that is independent of the 41-case dataset, an approach that is standard in verification work (Doswell and Schultz 2006, their section 5).

Other problems pertaining to the evolution of convective systems plague the quantities introduced by Qian et al. (2015). Relative vorticity and divergence associated with a mesoscale convective system would change as the system evolved, whereas the relative humidity term would be consistently close to 1 in

saturated regions (e.g., Fig. 7 in Qian et al. 2015). Also, there is the problem with the time scales, as the heavy precipitation area ($\geq 25 \text{ mm day}^{-1}$) is compared to these diagnostics at a single time of 0000 UTC; convective systems evolve on much quicker time scales, so there is an inconsistency between the diagnostics and the 24-h rainfall. The evolution of these quantities as a function of the phase of the life cycle of the convective system is not presented. For these various reasons, we question the utility of these parameters as valid meteorological quantities.

5. Why not just plot the model precipitation field directly?

At various points in the article, the parameters are said to "diagnose," "track," "capture," "trace," and "accurately depict" heavy rain locations. The article also refers to these parameters being used to "post-process model forecasts to improve predictions." The most basic question that should have been asked of Qian et al. before publication is, "Why are *other parameters* needed to track heavy rain locations in model output?" Why not just directly plot the model forecast rain maxima? Moreover, the authors have not demonstrated that their diagnostic is superior to the performance of the direct model output of precipitation nor have they addressed why looking at the precipitation field from the model is not worthwhile.

6. Qian et al.'s parameters are inconsistent with an ingredients-based approach

The authors state several times in their paper that these diagnostics are "not intended to replace a complete, multiscale forecasting methodology." If so, then what is the value of these quantities? Assembling arbitrary quantities to form a new index is an improper approach to developing diagnostic tools (e.g., Schultz et al. 2002, their section 3c; Doswell and Schultz 2006). In particular, Doswell and Schultz (2006) discussed the problems when combining two separate quantities that may not even be collocated and can evolve largely independently of each other. This problem underlies the parameters of Qian et al. (2015).

On p. 1413, Qian et al. justify moist vorticity and moist divergence in this way: "Since heavy precipitation is closely associated with convergent and cyclonic flows in a moist environment, either dynamic or moisture factors alone should not be able to accurately depict the area of a heavy rain event." In fact, heavy rain is a result of different ingredients coming together (Doswell et al. 1996). No single parameter can combine all of those

ingredients. Thus, the approach described by Qian et al. fails the conditions for being an ingredients-based approach because of its lack of considering all the necessary and sufficient conditions for heavy precipitation.

7. Conclusions

This comment has presented our concerns with the introduction of moist relative vorticity and moist divergence to track regions of heavy rain, as presented by Qian et al. (2015). These parameters fail in their mathematical construction, their meteorological justification, and their practical application. This comment also reflects our concerns with the similarly constructed and used GMPV parameter of Gao et al. (2004).

In this way, we have a dramatically different vision of the future than that articulated by Qian et al. who wrote, “Our final hope is that the approach of combining dynamic and moisture factors together as demonstrated in this study could inspire similar works in the future to advance our understanding of atmospheric behavior and improve diagnostic and prediction tools.” *We see the approach of combining kinematic and other atmospheric quantities in a scientific ad hoc manner as neither useful for improving understanding nor adequate to advance diagnostics and prediction.* We argue that diagnosis of heavy rain locations is best done by direct model output of precipitation (e.g., Roebber et al. 2004; Kain et al. 2006) or through an ingredients-based methodology (e.g., Doswell et al. 1996).

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